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Note on the preceding Article, By SIR EDWARD COLEBROOKE,
BART., M.P., PRESIDENT R.A.S.

HAVING been led from the interest which has attached to Mr. Colebrooke's opinion, on the subjects discussed in Professor Whitney's paper, to examine the very copious memoranda on Hindu astronomy, which are in my possession, I think I am in a position to clear up some of the points which have given rise to this controversy. It seemed to be so improbable that Mr. Colebrooke should have taken such pains to identify the constellations named in the passage of the *Jyotish*, and have so pointedly referred to the double observation of the solstices as determining the epoch of the observation, while he was really grounding his opinion on the position of another asterism, that I thought the subject merited a closer scrutiny.

I do not question the fairness of Professor Whitney's ingenious conjecture, in the absence of direct evidence, as to the opinions of these eminent scholars. It might have been allowable for Mr. Davis or Mr. Colebrooke to have contended in the manner here attributed to them, that the description of the solstitial colure as passing through the *middle* of Aśleshā was to be taken loosely, and that it might be reconciled with the position of *Kṛttikā* as representing the equinox. This suggestion on the part of Professor Whitney derives some support from another passage in the same essay on the *Vedas*,¹ where the origin of the zodiac at the beginning of *Kṛttikā* is spoken of as agreeing with the middle of Aśleshā , if the divisions of the zodiac are reckoned at twenty-seven equal portions, and its end if they are twenty-eight. Whatever may have been his opinions on this point, I cannot doubt, on reading the passage at the conclusion of

¹ *Miscellaneous Essays*, i. p. 90.

the essay, that his conclusion, as to the date of the calendar, is founded on the position assigned to the solstitial colures, and upon them only.

I do not propose to enter upon the wider question, which is raised in this paper—how far these ancient observations are to be accepted as approximative evidence of dates. The reasons adduced by Professor Whitney for doubting whether the lunar mansions represented equal divisions of the ecliptic at this ancient period, are very cogent, and must approve themselves to those who are conversant with practical astronomy. The margin which he allows for error (1000 years) seems far too wide, and I think it could be shown that a higher probability attaches to the twelfth or thirteenth century B.C., in the case before us, than to those which may be included within the limits of possible error. I confine myself for the present to the question discussed at the commencement of the essay, viz., the grounds of Mr. Davis' and Mr. Colebrooke's opinions.

Now on turning to Mr. Davis's paper on the Indian Cycle of Sixty Years (*Asiatic Researches*, vol. iii. p. 225), I find the following passage:—

“To render the paper more intelligible, I have subjoined a diagram of the Hindu ecliptic, which may also serve to illustrate some astronomical papers in the preceding volume. Its origin is considered as distant 180 degrees in longitude from *Spica*: a star which seems to have been of great use in regulating their astronomy, and to which the Hindu tables of the best authority, though they differ in other particulars, agree in assigning six signs of longitude counted from the beginning of *Asrini*, their first *Nacshatra*.”

The distance in longitude of the same star from the equinox in 1750 was, according to Herschell,¹ $20^{\circ} 21'$. At the rate of seventy-two years for each degree of pre-

¹ *Astronomy*, 381. Mr. Colebrooke's MS. memoranda gives the following calculation: “*Spica* in 1800, RA $198^{\circ} 40' 1''$, whence long. $21^{\circ} 3'$.” Allowing for the difference of dates this gives the same result as Herschell. I have compared these with several catalogues of stars of the last century, and they agree within a minute when reduced to the same date. It is probable, therefore, that Mr. Davis assumed a different rate of precession than that which is used in the text.

cession we are carried back to the year A.D. 285, when the star was on the equinox. Assuming that the equinox had receded one sign and three quarters since the epoch of the old observation, we are brought to the year B.C. 1395. This comes so near Davis's date (B.C. 1391. Asiatic Researches, vol. v. p. 288) as to leave very little doubt as to the grounds of Mr. Davis's opinion.

It is open to question whether Davis was justified in placing his reliance on this star in preference to Revatî. Professor Whitney assumes that only two courses are open to us, either to accept the observation of the latter as determining the origin of the Zodiac or to average the errors of all the junction stars. The objection to the latter course is that these stars cannot be said to be identified with equal certainty and the observations are of unequal value. No doubt has ever existed with regard to the two above named stars, which respectively represented the equinoxes, and were probably observed with greater care than many of the others. The place assigned to Spica by Davis is that of the Sûrya-Siddhânta, and is inconsistent with the position assigned to the same star in other works. But, on the other hand, it may be contended that the observation of one so conspicuous as Spica is of higher value than that of ζ Piscium, a star of the fifth magnitude, and barely visible to the naked eye. It will be seen presently that Mr. Colebrooke took the average of the two.

It is assumed in the preceding remarks that there was no question as to the time which was supposed to have elapsed since the old record of the position of the colures and the modern observations which determined the origin of the zodiac. It is inferred by Davis and his contemporaries, from the passages quoted from the writings of Varâha Mihira and other astronomers, of what may be called the modern school, that the equinox had receded one sign and three quarters since the ancient epoch, and that they were dealing with a division of the zodiac into twenty-seven and not twenty-eight asterisms. The precessional motion is therefore taken at $23^{\circ} 20'$, equivalent to a difference of date of 1680 years.

Confirmation of this will be found in Davis' diagram, already referred to. Lines are drawn marking the former position of the colures, and the equinox is placed at 10° of Bharanî, the position assigned to it in his former essay; other lines mark the limits of the precession, resulting from the Hindu method of computing it, alluding apparently to the supposed oscillation of the equinoxes to the extent of twenty-seven degrees on either side.

Turning now to Mr. Colebrooke's views, the evidence of his reliance on this observation of the star Spica is even more distinct than with regard to Davis. At the conclusion of an elaborate examination of the allowance for precession in the Benares Almanac I find the following remarks:¹—

“It is by Spica then that the Sûrya-Siddhânta's origin of Mesha is determined.

“Origin of Mesha according to the Sûrya-Siddhânta, taking the mean of the places of Revatî and Chitrâ $19^{\circ} 16' 30''$ from the equinox of 1800.”

The former passage is scored through, together with some calculations that precede it, and the latter left as his final conclusion. The inference seems irresistible, that he had at one time leant to the views of Davis, but he finally concluded that it would be safer to take a mean of the two observations.

The grounds of his reliance on the observation of Spica do not appear very distinctly from the papers before me. His aim was evidently to deduce from the very errors of the Hindu rules some conclusion as to the probable epoch of their formation. Thus it appears, from his comparison of the time of the vernal equinox given in the Benares Almanac with that of the Nautical Almanac for 1806, that there is a difference, after making allowance for longitude which, when reduced to minutes and seconds of a degree, amounts to $53' 18''$. Deducting this from the longitude, assigned in the same almanac to the sun on entering Mesha, the result agrees within one minute with the amount of precession calculated by the rules of the Sûrya-Siddhânta. This quantity of $53' 18''$ represented, according to his views, the accumulated error arising

¹ The manuscript from which I quote bears the watermark of 1808.

from the excess of the amount of yearly precession according to the Sûrya-Siddhânta over that assigned by modern science. The difference of equinoxes, $53' 18''$, divided by this excess ($3\frac{3}{4}''$), gives 853 years, which he thinks represents the period which has elapsed since the beginning of Mesha was determined where it now is. From calculations, similar in character, he arrives at another epoch of 1059 years. Intermixed with these calculations, which are worked out with some elaboration, are comparisons between the amount of precession, according to the rules of Sûrya-Siddhânta, with the longitude of Revatî according to the same work, and the position assigned to the same star by Brahmagupta. These again are compared with the longitude of Chitrâ (Spica) and the mean longitude assigned to the sun on entering Mesha. The evidence which this manuscript affords of the close scrutiny this question underwent will, I doubt not, prove of interest, though I am unable to trace the grounds of his conclusion with regard to the origin of the Zodiac. He was evidently struck by the fact that the longitude of Spica was but little in excess of the position assigned to the equinox in the modern astronomy of the Hindus. It is more so with regard to their allowance for precession. All the quantities exceed considerably the longitude of Revatî. His aim was to restore a curious chapter in the history of Hindu astronomy, but the notes are incomplete and partially erased, and he finally concluded that we should not expect precision on such a question. His distrust of the accuracy of the old observations is frequently expressed in his published works, and nowhere more strongly than in a short paper in the Asiatic Journal, for 1806, in reply to the strictures of Bentley.

I would invite those who may desire to pursue the subject further, to note the use he has made of astronomical data, in his attempt to determine the age of Varâha Mihira and Brahmagupta, in his work on Hindu Algebra.¹ In the former case he takes the mean of two observations of the star Chitrâ, viz., that of the Sûrya Siddhânta, and the position assigned to it by Brahmagupta. With regard to Brahmagupta

¹ Notes F and K appended to the dissertation.

he gives the date which might be deduced from that author's mention of the position of Chitrâ. This is compared with the position assigned to Revatî, and the mean of the two is given. This again is compared with the position assigned to Canopus and Sirius by the same author.

If the mature opinions of an author are to be gathered from his latest writings, it is clear that those of Mr. Colebrooke on this question of chronology are marked with the same cautious criticism which distinguished his other writings. The practical astronomy of the Hindus is described in his reply to Bentley as too loose and imperfect to be employed otherwise than as approximative evidence of dates, and the use he made of the materials before him was consistent with this principle. The only exception, if it be one, is in the reference to the Jyotish in his *Essay on the Vedas*. Could he have foreseen that he would be afterwards quoted in asserting that "the position of the solstitial points at the beginning of Dhanisthâ, and in the middle of Åsleshâ could have been a reality at no time except in the fourteenth century B.C.,"¹ he would probably have qualified the passage in question on the re-publication of his essays.² It is clear to me on reading this passage with the light thrown upon it by his other writings, that nothing was further from his thoughts than the dogmatism here attributed to him, and that no one would more readily have admitted that any inference with regard to dates must be taken with a very considerable margin for error, possibly to the extent of two centuries on either side.

At the risk of swelling this note to too great length, I add a remark on Professor Whitney's conjecture, that in the primitive astronomy of the Hindus the asterisms had no precise boundaries, but marked only that portion of the moon's path which was adjacent to the distinguishing constellation. It would follow that an ancient observer, when he referred to the beginning of an asterism, had in his mind only the boundary of the constellation. If we apply this to

¹ Max Müller, on "Ancient Hindu Astronomy and Chronology," p. 21.

² It is to be observed that the mean of Revatî and Chitrâ given above would carry us to the latter part of the thirteenth century B.C.

the record of the Jyotish, and accept Mr. Colebrooke's identification of the constellation Dhanishthâ, we shall find that the solstitial colure passed through the star β Delphini, which is the most western of the group¹, in the middle of the fourteenth century, B.C. Nothing so precise can be deduced from the examination of the opposite point of the ecliptic, for we have no such defined group as the Delphini; and Mr. Colebrooke and Professor Whitney are at issue in their attempt to identify the constellation Åsleshâ. Archdeacon Pratt assumes that Mr. Colebrooke could not have taken the constellations thus loosely, because the star ϵ Delphini and ζ Hydræ would only give an additional precession of 40' to his own previous conclusion. Had, however, he turned to Mr. Colebrooke's essay on the zodiac, he would have found that neither of these stars are included by him in the constellations in question, and that the star groups selected by him support the view of higher antiquity. The point deserves some attention, apart from any question as to Mr. Colebrooke's views on the subject.

¹ The right ascension of the star was, in 1800, $307^{\circ} 2' 3''$, according to Zach's tables. From this I make the longitude $313^{\circ} 33'$, which was the position of the solstitial colure about the year B.C. 1336.
